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Comments on "Shadow Pricing and Macro-economic Analysis: Some Illustrations from Pakistan"[1] by Squire *et al.*

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The paper is too long for conveying the message that shadow pricing used as a method of analysis in micro-economic issues of project selection is also useful for analysing macro-economic issues, such as foreign and domestic borrowing by the government, emigration, etc. Much of the methodological discussion in the paper is available in a readily accessible form in several publications of each of the co-authors. In contrast, the specific application of the methodology to Pakistani problems is much too cavalier.

While it is hard to disagree with the authors' claim that shadow pricing "constitutes a relatively *informal* attempt to capture general equilibrium effects" (p. 89, emphasis added), their depiction of traditional analysis is a bit of a caricature: essentially it sets up a strawman to knock down. After all in the traditional partial equilibrium analysis, the caveat is always entered that the results are possibly sensitive to violation of the *ceteris paribus* assumptions of the analysis, though often the analysts will claim that extreme sensitivity is unlikely. Analogously, the shadow pricing method presumes "stationarity" of shadow prices in the sense that they are "independent of policy changes under review" (p. 90). The essential point to be noted is that the validity of this assertion or of the "not too extreme sensitivity" assertion of partial equilibrium analysts can be tested only with a full scale general equilibrium model! At any rate this reviewer would not pose the issue as one of traditional partial equilibrium macro-analysis versus shadow pricing as an approximate general equilibrium analysis, but would prefer a description of project analysis as an approach in which a *macro-general equilibrium* model of a manageable size (implicit or explicit) is used to derive a set of key shadow prices which are *then* used in a detailed micro-analysis of projects.

The formulae for CRI, ARI, etc. are reproduced (with some terse attempts at their derivation) from other publications. This reader is not persuaded that the paper will be any less intelligible if just a brief description of the role of CRI and ARI and the procedure of their computation is given. If a derivation is to be given at

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[1]This paper appears as the first article in this issue.

all, it is preferable to start from the implicit definition of the social welfare function as

$$W = \int e^{-pt} [\sum_{i=1}^{N} U(c_{it})] \, dt$$

where p is the social rate of pure time preference and $U(c_{it})$ is the utility of individual i at time t. With the specific function $U(c_{it}) = (c_{it})^{1-n}/1-n$ one can then write

$$W = \frac{1}{1-n} \int e^{-pt} (\bar{c}_t)^{-n} (\sum_{i=1}^{N} d_i \, c_{it}) \, dt$$

where $\bar{c}_t = \frac{1}{N} \Sigma \, c_{it}$ = average consumption at time t and $d_i = (\bar{c}_t/c_{it})^n$. It then becomes transparent that $e^{-pt} (\bar{c}_t)^{-n}$ is a *discount* factor applied to the weighted sum of the *consumptions* of the N individuals ($\Sigma \, d_i \, c_{it}$). Hence the rate of fall over time of this discount factor is the consumption rate of interest and of course, d_i is the weight attached to ith individual's consumption in (undiscounted) social welfare at time t. Since W is social utility, to convert everything to the numeraire (public income) you need the value $\underline{v}$ of public income in terms of social utility.

The way in which values of CRI etc. for Pakistan are inferred is nothing short of ludicrous: "... Pakistan's draft Fifth Plan makes it quite clear that growth is the primary aim of the next few years, although equity is not neglected. This suggests a zero or very low value for the rate of pure time preference (0 – 1%), because an increase in this parameter reduces, other things being equal, the extent to which the weighting system favours growth. It also suggests a positive, but not large, value for the social elasticity of the marginal utility of consumption (n). It must be positive in order to reflect some degree of concern for income distribution. But it should not be taken to be very large, since the higher its value the higher is the consumption rate of interest (CRI)" (pp. 92-93)! I would prefer instead to say simply that such and such values for CRI, etc. have been assumed.

The last paragraph on page 94 is puzzling, first for the statement that Pakistan's income distribution is relatively egalitarian (egalitarian relative to what?) and for its cross check of the identification of CCL. If I understand it correctly, it depends upon v = 1.2 being derived independently of CCF. For if v had been set as $\frac{1}{CCF} = 1.25 \approx 1.2$, then it is no cross-check to say that $\frac{d}{v}$ equals exactly 0.8 at the mean level of consumption $\bar{c}$, since d = 1 by definition at $\bar{c}$ and with v= 1.25, $\frac{d}{v} = 0.8$ at $\bar{c}$! Incidentally, Table 2 (footnote 1) refers to *income* distribution while the weights d_i relate to the *consumption* distribution. It is another illustration of the cavalier manner in which the data are used.

The SCF presumably applies to the cases "where constraints on information, time, etc. may preclude estimation of border prices or where, as for some minor non-

traded goods, no specific conversion factors are available" (p. 95). If this is the case, the formulae on p. 96 which are based on *all* imports and exports (including those commodities for which specific conversion factors are known) cannot be justified. Be that as it may, the real clincher is of course that "we have assumed arbitrarily that the likely range of the SCF should be 0.85 to 0.95 and that within this range the most likely value is taken as 0.90" (p. 97)!

One could go on and on with a critique of the arbitrary adjustments made to various, already dubious data. Suffice it to say that the illustrations of the methodology would be far more telling in their effect if the authors had contrasted their method with the traditional approach in at least one instance. It would appear from the results presented that the conclusions would not be changed dramatically if one substituted the unsophisticated traditional value of unity to SCF, CCF, etc. instead of the tenuously derived values of the authors!

Finally, in calculating the benefits of foreign borrowing, the authors introduce the expected rate of inflation of Pakistan's imports and exports, presumably to reflect the fact that a debt denominated, say in nominal dollars, becomes less burdensome to service if there is a rise in the dollar value of a unit of Pakistan's domestic resources used in exporting or importing. Roughly speaking, for this to happen, domestic inflation in Pakistan has to be less than that abroad in traded commodities. Can this be assumed to hold for the indefinite future? Be that as it may, it will be helpful to the reader if it is stated that the second line of the formulae on p. 99 is derived from the first by assuming that *ip* is negligible!